scalpels, forceps, and scissors, being nearly all that is essential to a biological laboratory. These can be procured by anyone; and the student when thus equipped with Huxley and Martin's "Practical Biology" in his hands, need only look around for some of the most easily obtainable animals, upon getting which he can start work in good earnest.

In the descriptive portion of the work there is one point to which we cannot help referring, which is in connection with the circulation of the blood. It is an explanation, originally given by Brücke, we believe, of the manner in which the mixed arterial and venous blood in the single ventricle of the frog is distributed in such a manner that the venous blood mostly enters the lungs. "It fills (during the systole) the conus arteriosus, and, finding least resistance in the short and wide pulmonary vessels, passes along the left side of the median valve into them. But as they become distended *and less resistance is offered elsewhere*, the next portion passes on the right side of the longitudinal valve into the aortic arches." The words italicised by us are those which it is difficult to comprehend, for it is evident that if the pulmonary artery offers less resistance at the commencement of the systole, it will do so all through the revolution in proportion to the relative calibre of its capillaries and those of the system generally; and then there is no reason why the valve should flap back.

OUR BOOK SHELF

Rotomahana, and the Boiling Springs of New Zealand. A Photographic Series of Sixteen Views, by D. L. Mundy. With descriptive notes by Ferdinand von Hochstetter, Professor of the Polytechnic Institution of Vienna. (London: Sampson Low and Co., 1875.)

THE autotype illustrations which form the main feature of this handsome volume are triumphs of the photographic art, and reflect the highest credit upon their author, Mr. Mundy. The photographs are on a scale quite large enough to give one a satisfactory idea of the main features of the various scenes intended to be portrayed; and by the judicious introduction into most of the views of the human figure, a good idea of the scale of the photographs is at once afforded.

The remarkable region illustrated by Mr. Mundy's series of photographs lies just about the centre of the North Island of New Zealand, in the south of the province of Auckland. The culminating or rather originating point of the phenomena described, Prof. Hochstetter regards as the still active volcano Tongariro, in the north of the province of Wellington. From this volcano three lines of volcanic action are supposed to proceed in a north-easterly direction by Lake Taupo to Lakes Rotorua, Rotoiti, and Rotomahana respectively, the last-mentioned line proceeding inwards as far as the marine volcano Whakari, in the Bay of Plenty; this line also, near its source, includes the hot springs at the head of Lake Taupo, about forty miles to the north of Tongariro. Another line, which follows to some length the outflow of the river Waikato from Lake Taupo, is marked by the hot springs and steam jets of Otumaheke and Orakeikorako, on the river's banks, and those of the Pairoa mountain range. The third line of action forming eruptions of this kind is exhibited in the hot springs of Rotorua and the solfataras of Rotoiti, which terminate these specimens of volcanic action on land, being situated near the sea-coast. While all along these three lines evidences of volcanic action are visible in the shape of hot springs, solfataras, geysers, mud-lakes, &c., the chief interest centres in Rotomahana, where the most beautiful and marvellous effects of this action are displayed. Though on a much smaller scale, the phenomena greatly resemble those which are seen in such profusion in the Yellowstone region of North America.

Mr. Mundy devotes most of his photographic views to the illustration of the phenomena to be witnessed in and around Rotomahana. This is one of the smallest lakes in the region, being scarcely a mile in length and a quarter of a mile in breadth; it is 1,088 feet above the sea, and the temperature of the lake itself varies from 60° to 100° Fahr. On the margin of the lake are many boiling springs, and around it are a great variety of phenomena similar to those which are witnessed in Iceland and in North America. It is impossible in a few words to give any adequate idea of these phenomena, and we must therefore refer our readers to Mr. Mundy's beautiful illustrations, and Prof. Hochstetter's brief but clear descriptions. One of the photographs gives a fine view of Lake Rotorua, about twelve miles north of Rotomahana, and the last four are devoted to the illustration of Lake Taupo and the phenomena to be seen in its neighbourhood. Rotomahana, we may state, is about forty-five miles N.N.E. of Lake Taupo, and about double that distance from Mount Tongariro.

Lake Taupo lies at a height of 1,250 feet above the sea, and no bottom has been found at a depth of 200 fathoms. Prof. Hochstetter conjectures that its waters, which have only one visible outlet, the Waikato, but many tributary streams, has a subterranean outlet to the north. It is this, he believes, which gives rise to the curious phenomena which abound in the region to which Mr. Mundy's photographs refer; the water, after being heated by underground volcanic fires, generates high-pressure steam that forces its way to the surface, bearing the characteristics of the rocks with which it has come into contact: the New Zealand springs, we should say, are divided into two distinct classes, the one alkaline, and the other acid. Whatever may be the value of Prof. Hochstetter's explanation of the phenomena, there is no doubt about the value of Mr. Mundy's illustrations of a district which seems to be all that now remains of a once extensive active volcanic region. While as a collection of well-executed views of great interest the work deserves a wide circulation, to the student of geology it is of great value, as affording a far more satisfactory idea of an important feature of the physical geography of New Zealand than any mere description can convey.

Elementary Lessons in Botanical Geography. By J. G. Baker, F.L.S., Assistant Curator of the Herbarium at Kew. (London: L. Reeve and Co., 1875.)

A WANT has long been felt of a small text-book for the use of lecturers and students dealing with the distribution of plants on the face of the globe. A work of this kind necessarily contains a large amount of detail and a formidable array of plant-names. These features of the present little volume are less objectionable when its special purpose is borne in mind, viz., the instruction of gardeners; the various chapters into which it is divided being in fact the substance of a series of lectures delivered to the gardeners at Kew. A reference to these details would be out of place in a short notice; and the best idea will be conveyed by giving the author's final summing up, viz:—That each species has originated from a single centre; that species have originated in different parts of the world, and that the flora of any given tract depends largely on its geographical position; that a large portion of the present genera (or types which agree in structure down to minute detail) were in existence before the end of the Secondary period, and have passed through the very great changes in climate and the relative positions of sea and land that have occurred during the Tertiary period; and that species (or types which accord not in structure only,

but in vegetative characters—such as shape of leaves and arrangement of flowers) were dispersed in broad outline as at present, before present islands were insulated and the present general dispersion of sea and land worked out. The reader will find in the volume a very large amount of information on these subjects compressed into a small space.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*]

Ocean Circulation

HAVING carefully read Mr. Croll's papers in the *Philosophical Magazine* for September and October, I find in them the full confirmation of my statement that his "crucial-test" argument is based on the assumption of an equilibrium between the Equatorial and the North Atlantic columns; the words "to be in equilibrium" or "in order to equilibrium" being used over and over again to fix this as the essential condition of the computation by which the North Atlantic column is made out to be $3\frac{1}{2}$ feet higher than the Equatorial.

No reference to other passages in Mr. Croll's writings can countervail this fact. I pointed out at Bristol the fallacy it involves, which was at once recognised by Sir William Thomson, General Strachey, and other competent authorities. This fallacy becomes obvious in the following parallel case:—

The specific gravity of Ægean water being to that of Black Sea water as (say) 1029 to 1013, a column of Black Sea water 1,029 feet high would be required to balance a column of Ægean water 1,013 feet high; therefore (on Mr. Croll's assumption of an equilibrium) the level of the Black Sea must be above that of the Ægean in the proportion of 16 feet to 1,013 feet of depth. But that there is *not* an equilibrium between the two columns, is conclusively proved by the deep inflow of Ægean water which always accompanies the surface-outflow of Black Sea water, showing the Ægean column to be the heavier.

Now Mr. Croll has obviously no more right to assume an equilibrium between the North Atlantic and the Equatorial columns, and thereby to deduce from their relative temperatures the higher level of the former, and the consequent impossibility of the thermal circulation as making the poleward upper flow run uphill, than he would have to deduce the excess of level of the Black Sea from its lower salinity, and to assert that an inward underflow of Ægean water is impossible, as tending to raise that level yet higher.

But there is yet another serious error in Mr. Croll's computation, which, even admitting his fundamental assumption, completely invalidates his conclusion. He has entirely omitted the consideration of the *inferior salinity* of the Equatorial column; which, as it shows itself alike at the surface and at the bottom, may be fairly taken as characterising its entire height. This will make a difference *in the opposite direction* of about one foot in 1,026; sufficient, therefore, if the excess in the North Atlantic column extends to a depth of no more than 600 fathoms, to neutralise the whole $3\frac{1}{2}$ feet of elevation which Mr. Croll deduces from relative temperatures.

Mr. Croll is unable to see what the "viscosity" of water has to do with the question. Just this—that it affects his whole doctrine of "gradients." The nearer water is to a "perfect fluid," the less is the gradient required to give it horizontal motion.

If a viscous fluid be drawn from the bottom of one end, *A*, of a long trough *A—B*, its level at *B* will be lowered more slowly than at *A*, and will remain appreciably higher so long as the outflow continues. But in the case of a "perfect fluid" and a slow outflow, the level will practically fall simultaneously along the whole length of the trough *A—B*. I am quite aware that, *mathematically* speaking, the level must be always lower at *A* than at *B*; since there can be no movement of any particle from *B* towards *A*, unless room has been previously made for it. But if the time required for the replacement of each particle by the one next adjacent to it be infinitely small, the excess of reduction at *A* will also be infinitely small.

Now, according to the authorities I previously cited, water approaches so nearly to the condition of a "perfect fluid," that very small differences in its density will suffice (if constantly renewed) to maintain a vertical circulation, *without any appreciable difference in level*. And my position is, that the void created by the slow descent of water chilled by the surface-cold of the Polar area will be so speedily replaced by the inflow of water from the circumpolar area, and this again by inflow from the temperate region, as to produce a continual upper-flow of equatorial water towards the pole, without the gradient which Mr. Croll persistently asserts to be necessary.

I now leave it in the hands, not of Mr. Croll, but of competent authorities in Physics, to decide (1) whether his "crucial test" has the value he himself assigns to it, and (2) whether his doctrine of "gradients" can stand examination by the light now thrown upon it by Mr. Froude's researches. Until some physicist of equal weight with Sir George Airy and Sir William Thomson shall pronounce the doctrine I advocate to be untenable, I shall continue to believe, with Lenz, Arago, and Pouillet, that it is the only one which can account for the phenomena of Deep-sea temperature.

That the temperature of the upper stratum of the ocean is often affected by the Wind-circulation, and is especially thus modified in the North Atlantic, I have repeatedly pointed out. And it is scarcely fair in Mr. Croll, therefore, to continue speaking of the "wind-theory" and the "gravitation-theory" of Ocean Circulation as if they were antagonistic, instead of being not only compatible, but mutually complementary—the wind-circulation being *horizontal*, and the thermal circulation *vertical*.

As, however, Mr. Croll has now advanced so far as to admit that "physicists may differ from him in regard to whether or not the present difference of temperature between the ocean in equatorial and polar regions is sufficient to produce circulation," I am not without hope that in another year or two he may come to accept the Thermal-circulation as a "great fact;" and that he may then make good use of his knowledge and ability in elucidating the shares which are taken by the Wind-circulation and the Thermal-circulation respectively, in the distribution of terrestrial heat. WILLIAM B. CARPENTER

The Sliding Seat

MOST problems in animal mechanics are of so complicated a character as to be generally referred to direct experiment rather than to mathematical analysis.

In Mr. Wagstaffe's remarks (vol. xii. p. 369) on the analogy which exists between the movements at the sterno-clavicular articulation in rowing, and those permitted by the sliding seat, we have an argument in favour of the latter arrangement. But when the subject is regarded from the point of view assumed by a practical oarsman, the question of actual advantage still remains unanswered.

There are certain preliminaries which must be considered before we can commence to solve the problem, leading to its subdivision into several distinct problems, some of which will prove interesting to the anatomist, some to the mechanician, some to the physiologist. In the following remarks I shall attempt to indicate the preliminaries referred to.

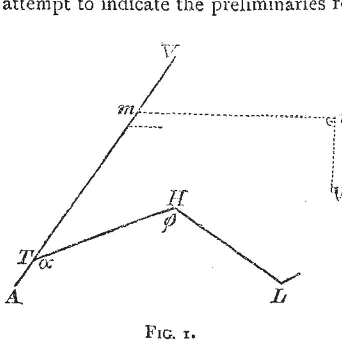
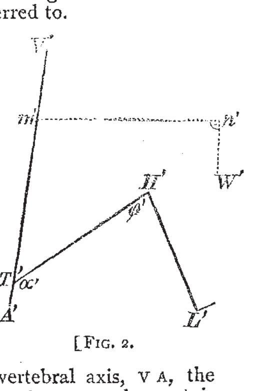


FIG. 1.



[FIG. 2.

Fig. 1 represents the position of the vertebral axis, V A, the thigh, T H, and the leg, H L, when the point A or the seat is fixed.

Fig. 2 exhibits the same parts when A' is movable. In both there is the same position for the outstretched arms, that is, $m\,n = m'\,n'$.

It is clear that in 1 the weight, W, will be raised by such forces as tend to move V A towards the vertical position; while in 2 the same result is obtained by changing V' A' without alteration of the angle of inclination. We thus see that the angles α and ϕ will vary in definite inverse ratio, while the varia-